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Discussions

Interactive comment on "Globally significant yields of dissolved organic carbon from small watersheds of the Pacific coastal temperate rainforest" *by* Allison A. Oliver et al.

Anonymous Referee #3

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Review of Oliver et al., "Globally significant yields of dissolved organic carbon 1 from small watersheds of the Pacific coastal temperate rainforest."

This paper describes the flux and character of dissolved organic carbon (DOC) from a series of small watersheds in the Pacific coastal temperate rainforest in British Columbia. The study provides valuable new information about DOC export from a relatively understudied region. The magnitudes of the watershed DOC fluxes are impressive; however the watersheds are very small so the statements about global relevance should be tempered accordingly. The extensive dataset on DOM quality could also be better utilized to understand the mechanisms that are driving DOM export rather than just make broad observations about streamwater DOM quality. Overall, I think this

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could be a solid contribution to the literature if these aspects of the paper are revised and strengthened.

General Points

There is no such thing as a "globally important" DOC yield; it is the total mass flux of DOC that could impact biogeochemical cycling on a regional or global scale. The yields reported here are quite high, however this is largely a function of the fact that DOC yields (flux per area) are inversely related to watershed size and the watersheds in this study are very small and have high wetlands coverage. Sampling a watershed that was smaller and had higher wetland coverage would produce an even higher DOC yield, however this would not make it more globally relevant. For this to represent a globally important finding, the authors would have to make the case that the fluxes measured here are broadly representative of the ∼100,000 km2 perhumid coastal forest in BC and thus provide evidence of a substantial mass flux of DOC to the coastal ocean. I understand that the purpose of this paper was not to calculate regional fluxes, however more directly addressing the issue of how regionally representative these high flux watersheds are would: 1) give readers a more concrete sense of the regional/global importance of these fluxes and 2) better justify statements such as "the small watersheds of this region export very high amounts of terrestrial DOC" (Line 477). The only place this issue is addressed in the paper is briefly in the conclusions (lines 552-554).

A similar issue arises in the discussion of the yields in section 4.1. Comparing DOC yields from these 3-10 km2 watersheds with yields from the Congo and Amazon doesn't make sense given the difference in scale. The Congo exports more than 10 Tg DOC/yr and all of the watersheds in this study together export probably 1/1000th of a Tg DOC/yr. The claim that DOC yields measured in this study are higher than those reported in southeast Asia should also be clarified given that Moore et al. (2011, 2013) have reported DOC yields >2x those reported here for watersheds in Indonesia that are several orders of magnitude larger than the watersheds in this study (doi:10.5194/bg-8-901-2011; doi:10.1038/nature11818).

There is some discussion material mixed in to the Results section of the paper. Examples include: Lines 336-339 and 345-347.

This is a very rich data set in terms of DOM compositional information. That said, the compositional data were somewhat underutilized in the study. For example, the 13C data were not even mentioned in the Discussion. In addition, the stream gage data are not utilized to elucidate how streamflow impacts DOM quality. Instead there are general statements about how compositional data change between wet and dry seasons (e.g. lines 445-456).

In Fig. 3 it appears that streamwater DOC concentrations are correlated with air temperature. If this is the case it would suggest that there is a link between soil temperature and soil water DOC production that influences the export of DOC to streams. Thus, temperature may be useful for predicting seasonal changes in streamwater DOC concentrations.

There are a number of references to watershed residence time in the Discussion (for example, lines 433, 492, 502), but it is not clear how this was quantified and whether it was function solely of lake influence or if watershed slope played a role as well.

Minor edits:

Line 74: The phrase “predictions of ecosystem productivity and food webs” is extremely vague

Lines 100-101: How and why would you expect DOC export from perhumid forests in Alaska to be different from perhumid forests in British Columbia? In other words, is there a reason to think that the work done in Alaska would not be valid in the same forest type in British Columbia?

Lines 104-105: The fact that discharge was directly measured is a strength of this study, however it is somewhat misleading to compare this highly localized study to continental and global scale studies where modeling discharge is a necessity.

Line 273: It seems redundant to report climatewna data in the study site and in the results. Also the values reported for mean annual precipitation differ between the study site (line 115) and the results (line 273).

Line 278: The comparison of precipitation at the study site to "most regions of the world" is vague and does not illustrate anything meaningful.

Lines 291-295: This sentence is repetitive and very hard to follow with all of the parenthetical data references. Recommend simplifying it to make the point about the difference in wet season flow without all of the Q data. It is also interesting that wet season Q differed by >20% between the two years while wet season precipitation only varied by $\sim$5%.

Line 326: It would be more clear to say that SUVA values were at the high end of the range rather than "relatively high compared to the range".

Line 417: "Catchment" looks like it should be plural.

Line 419: The term "a significant biogeochemical hotspot for coastal carbon cycling" is somewhat vague. Many of the studies cited in this paper calculate end of pipe DOC fluxes "directly to the coastal ocean". It would be helpful to more specifically explain why the watershed DOC fluxes in this study are "significant" from the standpoint of the coastal C cycle.

Lines 425-6: Does the term "high precipitation event" refer to intensity or magnitude. Also, it seems like the slope of these watersheds (typically >30%) is an important factor in the short hydrologic residence times that is not mentioned in this paragraph.

Lines 430-431: I agree that seasonality is important for ecological processes and it would be helpful to provide more analysis about why this would be the case in this region.

Line 455-456: Again, the consequences should be explained or this sentence should be removed.

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Line 546: Because yields are a measure of the per area export (flux) of DOC the term "export the highest yields" is redundant.

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